Boston University

Department of Physics
590 Commonwealth Avenue
Boston, Massachusetts 02215

Office of the Chairman

617/353-2600
Telefax 617/353-9393
Telex 286995 BOSUNIVBSN
Bitnet SULAK/BUPHYA or BUPHYC

Dept. 22

Dear Robert,

I hasten to write to you, Michael, and Jeremy, because any suggestions I make now may save you much time in revising the next version of your paper. I do not have my last letter to you infront of me, but I believe that I remember its contents pretty well. In particular, I don't think I said that we are in complete agreement on the issues raised. What I recall saying is that I have sufficiently high regard for the contents of your paper to accept it even if you don't make all the radical revisions that I would like, provided that you made the paper more readable and corrected some points which seem to me to be unequivocally mathematical errors. I really would like to see you make more than the minimum revisions needed to satisfy me as referee, because I think your contribution to the literature would be greater if you did so If I comment on your letter of Sept. 14, you may see more clearly the loci of our disagreement, and then you can decide how you should rewrite.

My answer to question A on p. 1 is unequivocally 'yes', and I thought that we made this clear. Look at the proof on pp.12-3 of the GHSZ paper. The angle ϕ is an arbitrary real number. Given ϕ, four settings are chosen satisfying (11a), namely, those of (12a-d). Thus, using rigorous set theory and measure theory, we obtain Eq. (16) for that specific ϕ. But since ϕ is arbitrary we have proved that the result function $A_\lambda(x)$ is indeed a constant function in the real variable x, and since the domain of x is the real numbers, we have proved the result for uncountably many values of x. It seems to me that you confused (both in your paper and in your letter) the question of the uncountability of the domain with the question of the uncountability of the number of settings needed to proved Eq. (16) for a specific ϕ. The proof in GHSZ is presented at the level of rigor common in mathematical (not just physical) theorems on functions of a real variable. Of course, real variable theory can be and has been formalized, but clearly it is inappropriate for us to work with a formalization, and neither do you. I fear that I become some what testy about this matter of rigor, because I thought I had made my point twice already -- once in the letter to you, once in the referee report. In your defense I might say, however, that in the original GHZ paper the matter is not so clear. One of my contributions to the four-man paper was just this clarification.
The answer to B is implicit in what I have just said: for a specific ϕ one does not need uncountably many settings, since four will do; but different sets of four settings are used for different angles ϕ, and there are uncountably many of these. I make this distinction in order to clarify your question.

In the second paragraph of p.2 you say that you are not aware of any "interesting" measure-theoretic questions that arise in your proof. Nor am I. But I know that there are interesting measure-theoretic questions elsewhere in physics, and I thought you might want to make a tangential remark about these, in order to assert the moral that care is sometimes essential in measure theory.

I do agree with the third paragraph on p.2, that you have interesting new things to say about unpacking "locality", and I believe that I encouraged you in the referee report to stress your innovations.

Some of the points in your fourth parapgraph do not seem to me to be innovations, though I have no objection to re-establishing results in new ways. It seems to me

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that GHZ and GHSZ essentially already had (i) and (ii), and in fact they credit Bell's 1964 paper with deriving determinism rather than postulating it (as many misinformed commentators have said). Points (iii) and (iv) I have addressed in my second paragraph above. Points (v) and (vi) may be right, but precision is needed in exhibiting your innovations. As you note in paragraph 2 of p.3, you find agreement concerning the inequality for a three particle system, and it is interesting to see different ways of obtaining it.

There are some differences between us concerning the matters of parag. 3 of p.3. We continue to hold that nothing experimentally interesting arises if premiss (i) (the quantum mechanical prediction of strict correlations) is dropped. Of course, if something else takes the place of premiss (i), then interesting conclusions can be drawn, and that is what we do on p.19, where we assume the existence of the result functions (i.e., we accept EPR's program without using EPR's argument). Could one substitute something else? Yes. For instance, if experimentally one found perfect correlations, one wouldn't have to draw on the quantum mechanical predictions in order to assert them. But our remarks about the imperfection of apparatus -- the fact that most quadruples are not observed, and the possibility that some of them are observed incorrectly -- show that this option is not feasible. I don't think that the "curiosity of the logical situation" was the motivation of our postulation of the result functions; the motivation was the impossibility of deriving anything experimentally significant from (ii), (ii), (iv) alone, without some supplementation.

You may be right that we relied too much upon the equivalence theorems of Fine and Stapp and that you may have something stronger than what we have with the help of their theorems. Hence, I think you ought to present the argurment of paragraph 1 of p.4 in full detail.

You claim that Zeilinger agreed with you in Finland about the proof of the constancy of the functions of $\emptyset$. I can only say that I think that not only you, but my collaborator, are wrong. When I next see him, I shall argue with him about the point. Writing an article in collaboration is an exercise in dialectic, to use Plato's terminology, but it may also be characterized as a cat fight!

I look forward to seeing you at the PSA meeting. Give my best greetings to Jeremy and Michael.

With best wishes,

Abner

Abner Shimony